

angle of which varies, and it becomes almost impossible in a broken equatorial, in which the eyepiece is independent of the moving part of the instrument, as proposed by Mr. Grubb. In a word, taking no account of the new and very grave causes of variability introduced by Mr. Grubb, this optical system is so unstable that its employment has been rejected unanimously by all astronomers and opticians. The least derangement of the position in the central mirror spoils everything.

The mobility of the plane mirror presents equally, from the optical point of view, a slight inconvenience. The quantity of light varies with the different angles of inclination, which renders the exact researches of the photometric very difficult. Without making one feel all its gravity, Mr. Grubb has, in truth, indicated the defect of this optical system. But in order to turn the difficulty he suggests that, since the field of view becomes smaller as the instruments become larger, we may content ourselves with observing at a central point. But this is an affirmation pure and simple. It is necessary in many measures of precision to have a large field of view. The contrary will present several serious objections. We have, in fact, to observe stars in relation with other stars, to measure, for instance, the difference of declination between a planet and a star of comparison. But we cannot make both these observations at the centre. The same thing will always be happening, in the case of comets, nebula, and clusters. It seems to me, on the contrary, that a telescope is more perfect the larger the field of view. Feeling thus, I have had made by Prazmowski, for my new equatorial *coudé*, achromatic eyepieces giving a very large field. For the observation of comets I have such an eyepiece, which magnifies fifty times and has a field of view such that I can observe a degree. For a telescope of twenty-seven inches we might have such an eyepiece with a field of twenty-four minutes.

From all which precedes, I think everybody will agree that the system proposed by Mr. Grubb is far inferior to that now employed in ordinary equatorials.

(2) The mechanical point of view. The instrument consists actually of an ordinary equatorial, in which the part which carries the eyepiece is replaced by a counterpoise. It presents therefore, from the point of view of stability, all the defects of the ordinary instrument. Additional causes of instability inherent to the design are—

The micrometer and eyepiece are completely independent of the principal mass, which necessarily gives rise to different defects of decentering between the separated parts. Moreover, in this instrument there are three distinct movements. In addition to declination and right ascension, there is a third, which consists of a differential movement round the axis of the mirror. This last one constitutes alone, from the point of view of stability, a complication which does not exist in the ordinary instruments. All the movements of transmission are broken at a right angle, and are four times as long as those of an equatorial *coudé* of the same size. There will therefore be such loss in transmission, one would never have the instrument perfectly adjusted and oriented. We see, from the mechanical point of view, there is such an incoherence between the different parts of the apparatus, that it is inferior to those now employed. And it really cannot be compared with mine, which is almost as stable as a transit instrument, and in which the movements of transmission are excessively simple.

(3) If we examine Mr. Grubb's instrument from an astronomical point of view, we see it is based on a principle which no astronomer can admit, namely, that it is superfluous to observe the greater part of the northern heavens. In many investigations among the most elevated in the astronomy of precision—let us take stellar parallax as an example—one is obliged to combine observations made at different epochs of the year, and it is only by the combination of measures thus obtained that the desired result is arrived at. These stars must be observed, therefore, in the northern part of the heavens as well as in the others, for the vicissitudes of climate do not permit the astronomer to observe exactly how and when he wants. The same necessity presents itself in the study of the double stars; to ascertain and to discard the systematical errors in the angles of position the astronomer is obliged to observe these stars in all the celestial regions. If one wishes to limit one's self to the exploration of one side of the heavens, one would lose precious opportunities and gratuitously introduce serious difficulties. There are also many cases in which this choice is not possible. Thus, if we wish to discover new comets every part of the heavens must be explored, and if one wishes to observe them they must be observed where they are.

Finally, permit me to ask Mr. Grubb how he is going to study that part of the heavens which lies between 20° from the zenith and the Pole. This region of space, I take it, would be entirely closed to the observer with Mr. Grubb's arrangement. Any research, therefore, which touched the stars covering this large area could not be undertaken.

The independence of the micrometer of the rest of the instrument renders impossible any measures of precision. The orientation of the micrometer, in fact, is the fundamental base of every measure, and to do this preliminary work properly three or four successive operations have to be performed, and take the mean of the readings and adjust the apparatus by means of the circle of position. But this fundamental operation cannot be performed on Mr. Grubb's instrument. In fact, in practice, if one wished to take an angular measurement with this instrument, one would have to proceed somewhat in this wise: First of all it would be necessary to content one's self with one approximation as to the orientation; then to repeat this after every individual measure; and lastly to take into account the disorientation of the micrometer, to submit the readings of the circle of position obtained to fastidious computations with a view to compensate them. This gives an idea of all the inextricable complications in which one would find one's self involved in this case. In fact, to secure a simple observation of a comet it would be necessary to increase the readings and the calculation by four times, and after all one would only get a result inferior to that furnished by an ordinary equatorial. I don't believe there is a single astronomer in the wide world who would undertake observations of precision under such conditions.

It is quite true, as Mr. Grubb indicates, that the *oculaire* might be connected with the rest of the instrument, but then, new inconveniences of another order would arise. These, however, I will not discuss now, for, as I said at the beginning, Mr. Grubb's actual proposal is now alone in question. However this may be, I consider the conception of this equatorial is so defective, taken as a whole, that I do not think its adoption would be seriously recommended. Nor do I think that the project will go beyond its present stage, unless essential modifications are introduced, and in this case the instrument would become like my own.

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M. LEWY

Dust-Free Spaces

I VENTURE to call attention to some points in connection with the observations on "dustless spaces," &c., as detailed in the report of Dr. Lodge's lecture published in *NATURE*, vol. xxix. p. 610.

Certain observations and studies of my own lead me to think that, if attention be given to the points to which I wish to call the notice of physicists, results of the highest importance may be reached by means of the method of experimenting developed by Dr. Lodge and Mr. Clark, and described in the report referred to.

Dr. Lodge's statement (p. 611) that "cloud spherules are falling, but falling very slowly," is true when these spherules are not at a higher temperature than the atmosphere in their neighbourhood. When, however, very small particles floating in the air become heated, they warm the air immediately surrounding them, and then these particles are either buoyed up by a small envelope of heated and dilated air clinging to their surfaces, or they are borne aloft by the local currents which they create by contact with the surrounding atmosphere.

Observations continued for nearly fourteen years have convinced me that in ordinary clouds these two methods of lifting are combined—that to a certain extent each of the spherules or very many of the spherules of clouds are buoyed by adherent heated and dilated air, and that the whole of the cloud, in many cases at least, becomes warmer than its neighbourhood in general, which adds to its buoyancy as a mass of intermingled air, water, and vapour.

These remarks apply also to small particles of matter other than water. The action is the same except in degree. The very high specific heat of water enables it to heat surrounding air more readily and quickly than other substances do, and as a consequence masses of water as in clouds are lifted more quickly and to a greater height than masses of other bodies having the same proportion of surface to weight.

If it be remembered that radiant heat passes uninterruptedly through air, *i.e.* that air is diathermous, it will be seen that radiations from a distance striking upon particles of athermanous bodies suspended in the air will cause these latter to heat the

air about them, and produce upward currents or a buoying of the athermanous particles by dilation of the air in contact with them.

This affords a complete explanation of cloud-flotation and the flotation of fine dust-particles. For some years I have been in the habit of watching clouds, and by the use of the above theory have very often been able to account for forms, dimensions, and movements which I could not otherwise explain. Some four years ago I explained the above ideas to the Chief Meteorological Officer of the United States Signal Service, and received from him suggestions which have since afforded me the means of much pleasure in observing the locations and forms and movements of clouds, and although these irregular masses are subject to many complicating circumstances, I have never yet observed anything tending to weaken this theory of flotation, but have made many hundreds of observations tending to confirm it.

I trust that it will be taken for granted that I do not wish to attack the hypothesis of Dr. Lodge and Mr. Clark, that heated bodies "bombard" and drive away approaching particles. My object is simply to show that, as it seems to me, the theory of particles buoyed up by a locally heated fluid, when considered in connection with well-known principles of radiation, &c., is sufficient to account for the phenomenon of the "dust-free coat" described in the article alluded to.

Referring to the figures on p. 612, an ascending current is shown in the neighbourhood of the pipe or rod in Fig. 1. The theory which I have sketched would indicate that this current had been set up in great measure by the *indirect* action of the heated tube or rod upon the surrounding air.

I should contend that the dust-free coat may be explained as follows:—

A given particle which may be assumed to be directly below the rod is heated by radiation from the rod. It in turn heats and expands the air in contact with it; the particle with a coat of adherent air becomes lighter than the surrounding atmosphere, and the mote, with its jacket of expanded air, ascends towards the rod. As it reaches the point marked "slow moving" in the figure, it begins to find itself in air which has been heated directly by contact with the rod, and distributed near it by the small "circular" currents which always surround a blunt obstacle in a stream of fluid. At the outer limit of the "dust-free coat" the particle or mote is arrested because it has come to a point where the air is so warm that the mote can no longer heat its jacket enough hotter than its surroundings to cause buoyancy. It is arrested because it has reached a point where the surrounding medium is as light as its own air-float, much as cork is arrested at a surface of water.

The mote with its warm air jacket could ascend through cool and therefore heavy air, but the air warmed by contact with the pipe is too light to float it.

The dark "tail" above the rod, or tube, is the upstreaming dust-free air, warmed by the tube, and too light to carry motes, or in which motes have not been carried by any current.

The report of the lecture contains within itself some very striking confirmations of this theory. For example, Dr. Lodge tells us that at a high temperature the dust-free coat is thicker than at low ones. This is according to the theory of flotation as above set forth, because an approaching mote would sooner meet the increased body of air warmed by contact with the tube to a point sufficient to destroy the buoyancy of the mote and its jacket. Again, hydrogen is a light gas having a very high specific heat; hence according to this theory the mote would need more heat and more difference of temperature to float than in air, and consequently should not be able to float up to as near the rod. Now, Dr. Lodge states that "in hydrogen it [the dust-free coat] is thicker than in air." With a surrounding medium of carbonic acid, less heat and less flotation are required for the mote, as the gas is heavier and of lower specific heat, and, quite in accord with the theory, the dust-free coat "is thinner" than in air or hydrogen. Again, Dr. Lodge states that the dust-free coat is set up by a "difference of a degree or two," and it would apparently require a much more complicated theory than the simple one here advanced to account for this on the bombardment hypothesis, as the action has been shown to be—

- (1) Affected by the medium as to thickness of coat.
- (2) Obtainable at different temperatures in the rod.
- (3) Apparently dependent, not on the actual temperature of the rod, but on the differences in temperature between the rod and its surrounding dust-containing fluid.

The behaviour of cool rods or plates, as stated, is also in accord with this theory. A mote coming within the influence of the plate or rod is cooled by radiation and loses buoyancy in its air jacket. If above the plate, it therefore falls upon it; if below, it drops away. Dr. Lodge does not explain how a cool plate "bombards" the motes and drives them away from its lower side. If clearly explained, the method of experiment developed and now under study by Dr. Lodge and Mr. Clark, and that of Mr. Aitken on the condensation of water about nuclei, will probably be found productive of results of the very highest importance.

Questions of climate, rainfall, healthfulness of districts, fogs, mists, humidity, &c., can probably be better studied than in any other way by some form of apparatus based upon results obtained by these experiments, if the theory of flotation above set forth is connected with them, as I trust it may be.

EDW. W. SERRELL, Jun.

Chabeuil, Drôme, France, April 27

Mr. Serrell is no doubt perfectly correct in his view that the average specific gravity of a warmed and vapour-filled cloud may be often less than that of air. The ascent of the so-called "steam" from a kettle proves this, and he will find the view clearly stated in Maxwell's "Heat," p. 280. I did not enter into details in the Dublin lecture, but I was fully convinced of the truth of this statement.

His supposition that the dusty air near a hot body gets warmed not by gaseous conduction from the hot body but by interception of its radiation by the suspended particles, is not an unnatural one, but it is practically untrue. It is disproved by the fact that the concentrated radiation from the electric light is much less effective in warming dusty (or any other) air, than is the neighbourhood of a warm solid only a few degrees above the atmospheric temperature.

Mr. Serrell's criticism, that we do not clearly explain the down-streaming dark plane from a cool body observed by Lord Rayleigh, is quite legitimate. So far as I entered into the matter at all, I intended to indicate provisionally a distinction between a cool body and a very cold one—the boundary coming somewhere, say, between ten and thirty degrees below the air, or possibly depending upon actual temperature as well as on difference. I am not prepared to assert that the bombardment of particles towards a cool body begins the instant it is colder than the atmosphere. I think it possible that there may be a neutral point below which it begins.

But Mr. Clark is working out this among many other points, and I am not sure that his view at present agrees with my hypothesis. He will doubtless make a complete statement when he publishes an account of the quantitative research he is now engaged in. Till then I prefer to leave the account of cold bodies a little vague.

O. J. LODGE

The Supposed Volcanic Dust Phenomena

THE reddish circle round the sun, which I suppose must be considered as a kind of very large corona, alluded to by E. Divers of Tokio (*NATURE*, vol. xxix. p. 283), G. F. Burder (p. 525), and other observers, was invariably visible here, when circumstances favoured, from November 1883 up to April 3. In the middle of that day, and of the 4th, though circumstances seemed favourable for seeing it, no tinge of red was perceptible; but it was visible late in the afternoon of the 4th. Since then it has become more visible again, and from April 21 has been very plain, though not so conspicuous as it was originally. It is red in the middle of the day, and brown towards sunset, the bright space between it and the sun being blue or greenish.

The semicircle opposite the sun is now far fainter than it was originally, indeed I do not think I should notice it now without looking for it. It is now plainest when the sun is a little above the horizon, which was not formerly the case, and I have not seen it after sunset lately. This may perhaps be owing to a change in the height of the volcanic dust, or whatever it is.

The amount of sediment in the rain strikes me as being very large. I have at different times in the last few months collected it upon glass and examined it with the microscope: there appear in it a considerable variety of crystals and other transparent objects. Some of the crystals are like those drawn by Mr. Beyerinck (vol. xxix. p. 309). I have usually found a number of irregular transparent pieces, but I cannot say that they have